

# THE Sidereal Messenger.

*Conducted by Wm. W. PAYNE.*

Director of Carleton College Observatory.

MARCH, 1883.

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To impress upon the mind the reality of the perfection of the works of the omnipotent, the living GOD.—Professor JAMES C. WATSON.

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# The Sidereal Messenger.

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"In the present small treatise I set forth some matters of interest to all observers of natural phenomena to look at and consider."—GALILEO, *Sidereus Nuncius*, 1610.

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THE PARALLAX OF  $\alpha$  LYRÆ AND 61 CYGNI\*. By ASAPH HALL, of Washington, D. C.

The question of the parallaxes of the fixed stars, or their distances from the earth, was for a long time a subject of speculation among the ancient astronomers. Very little, however, could be done towards answering this question until the true theory of our solar system had been discovered, and the invention of the telescope had made possible more accurate astronomical observations. Even when these things had been done there remained other questions to be solved, such as those of aberration and refraction, before astronomers could reduce their observations correctly and bring the problem of stellar parallax to the test of exact measurement. All these reductions are easy to us now, but the astronomer who lived two hundred years ago found himself in a maze of perplexities, and nearly a century passed away before these difficulties were overcome so far as to make feasible the determination of the distances of the stars.

When it was seen that the motion of the planets around

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\*Read at Montreal meeting before the A. A. A. S.

the sun is undoubtedly the true theory of our solar system, and that we have a long base line to measure from, astronomers were not slow to undertake observations for stellar parallax. One of the earliest observers was ROTHMANN, astronomer for WILLIAM of Hesse, in Germany. Rothmann found a difference of  $2'$  in the latitude of his observatory from observations made in summer and in winter, and hence he inferred that the stars have an annual parallax, which confirmed the Copernican theory that the earth is in motion around the sun. But TYCHO BRAHE, the best observer of those times, declared that his own observations, made with the best instruments and with the greatest care, showed no such change of latitude; and hence TYCHO drew the conclusion that the stars have no parallax, and that the theory of the motion of the earth around the sun is false. One of the most ingenious early attempts to investigate stellar parallax was that of Dr. ROBERT HOOKE of London in 1669; but HOOKE seems to have been a man of unsteady purpose, and failed to carry out his plan of observation, so that his work came to nothing. The next remarkable attempt was by the English astronomer, FLAMSTEED, in 1689-1697. FLAMSTEED made very careful determinations of the zenith distances of *Polaris* at its upper and lower culminations at different times of the year, and correcting these for precession he found that the results showed a periodical change, which he ascribed to parallax. But as soon as FLAMSTEED published his observations, it was pointed out by CASSINI and others that the periodical changes could not be explained by a parallax of the star. It is curious, however, that no serious attempt was made to account for FLAMSTEED'S results, the most careful and accurate that had been obtained up to this time. These results contain, in fact, the data for a very good determination of the constant of aberration, as was shown by C. A. F. PETERS in 1846, and the value of this constant found from FLAMSTEED'S observations is in error only a quarter of a second of arc.

Another generation of astronomers continued their attempts at the determination of stellar parallax, from observations of the zenith distances of the stars, but with no

results. Many of these observations show periodical changes, which gave rise to various theories; but probably all these changes may be explained by variations in the instruments, or by incomplete reduction of the observations. In fact, no trustworthy determinations of parallax were possible until after the discovery of the aberration of light; and BRADLEY'S discovery of this phenomenon, in 1728, put an end to most of the crude attempts. The coefficient of aberration is  $20''.4$ ; and although the correction for aberration is zero at the time of the maximum of parallax, yet the motion of the earth in a single day will introduce a correction for aberration amounting to one-third of a second of arc, a quantity comparable with the parallaxes of the stars. BRADLEY was one of the few astronomers of the past century who knew the value of accurate and definite observations, and from his own work he drew the conclusion that the parallaxes of the stars must be less than a second of arc. BRADLEY'S labors seem to have shown astronomers the great practical difficulty of this problem of stellar parallax, and few further attempts were made until toward the beginning of present century when the instruments had been much improved.

The Italian astronomer, PIAZZI, in 1792-1804, found parallaxes of several of the bright stars, amounting in some cases to  $3''$  or  $4''$ , but his results are certainly wrong. About this time BRINKLEY, the astronomer Royal of Ireland, devoted fourteen years of careful and intelligent observation to the question of stellar parallax. This is one of the most remarkable researches of the kind ever made. The values of parallax found by BRINKLEY amount in some cases to  $2''$ ; and his work led to a long dispute between himself and POND, the astronomer Royal at Greenwich. Although BRINKLEY was a skillful astronomer, and a man whom no amount of labor could deter, yet we must say that his results for parallax are mostly erroneous. However, the great difficulty of dealing with this question, by means of absolute measures made with divided circles, seems to have been brought out clearly by BRINKLEY'S observations and their subsequent criticism and discussion.

The most elaborate and successful attempt that I know of to determine the parallaxes of the stars by such absolute measurements is that made by C. A. F. PETERS at Pulkowa, in 1842-'43. In the care and skill of observation, and completeness of reduction, this work has never been surpassed. PETERS finds positive values of the parallaxes of seven or eight stars observed, and in the case of the single negative parallax the value is less than a tenth of a second. The parallax of 1830 GROOMBRIDGE found by PETERS is  $0''.226 \pm 0''.141$ ; a value much less than that found by a differential method, and subsequent investigations have proved the correctness of PETERS' result.

The absolute determination of so small a quantity as a stellar parallax is an extremely difficult matter, since the observations must be extended over a year, during which the instrument is exposed to great changes of temperature which act on the circle, and which may influence the reductions. The divided circle may be avoided by using a transit instrument in the prime vertical, although the field of observation is in this way restricted to fewer stars. If STRUVE's method of reversing the instrument in each vertical be used, and stars are chosen that pass near the zenith; two of the instrumental corrections are almost wholly eliminated; but the third, or level correction, comes in with nearly its full value. With the excellent spirit levels made by REPSOLD, it was found by STRUVE that the probable error from this source was only a few hundredths of a second of arc. For stars of the first and second magnitudes that can be observed at all seasons of the year, this instrument promises good results, and in the absolute determinations of parallax, and in the investigations of the constants of aberration and nutation, it may yet fulfill the expectations of STRUVE.

It was long ago pointed out by GALILEO, that if two stars are near each other, and one of them is bright and the other faint, probably these stars are at greatly different distances from us; and that differential observations of such stars would lead to a knowledge of their difference of parallax. HUYGENS applied this method to some of the binary



systems, and of course found no parallax. A century afterwards HERSCHEL also applied this method to some of the binary stars, and again no parallax was found; but by continuing his observation HERSCHEL found that many of these stars were in motion around each other, and recognizing that such stars must be at nearly the same distance from us, he saw that no parallax could be found from such observations. In some respects, however, this method of GALILEO presents great advantages. The observations being merely differential they can be made with micrometers of various kinds, and with greater accuracy and ease than absolute determinations. The reductions are also far simpler and more easily computed, and we avoid in a good degree the effects produced by changes of temperature. It is true that by this method we get only the difference of the parallaxes of the stars, and there will always remain some doubt concerning the value of the real parallax. But if a number of different stars be used for comparison, and the same value of the parallax be found from the various stars, we may fairly conclude that the parallax belongs to the principal star. A practical difficulty is met with here in the fact that most of the stars that have large proper motions, and those that for other reasons are such as we would observe for parallax, have no suitable comparison stars within reach of the micrometers commonly in use; that is, if we follow the method of measuring angles of position and distance, which is perhaps the best. In some cases this difficulty may be avoided by observing only the difference of declination, when the coefficient of parallax in declination is not too small, although by doing this we give up the advantage of an independent determination of the parallax from the angle of position.

In 1862 a series of observations of  $\alpha$  Lyrae was begun at the U. S. Naval Observatory by Professor J. S. HUBBARD, and this series was continued by Professor S. NEWCOMB and his assistants until April, 1867. Each complete observation consists of four transits over seven wires, the instrument being reversed in each vertical. This star passing only a quarter of a degree south of our zenith, the correc-

tion for azimuth is nearly zero, and the collimation being eliminated by the method of observing, there remained only the correction for the level, which was determined by an excellent instrument made by WURDEMAN of Washington. This long and careful series of observations proved, however, of no value for determining the parallax of this star. Some unknown disturbance acted in such a way that the parallax came out negative. This unfortunate result led me to make an attempt to find the parallax of this star by the differential method, while our observatory remains on its present site. These observations were begun May 24, 1880, and were finished July 2, 1881. As the famous star 61 Cygni could be observed without spending much more time, observations on this star were begun Oct. 24, 1880, and ended Dec. 7, 1881. These observations were the simplest possible, being measures of the difference of declination from a comparison star of the tenth magnitude. My observations of  $\alpha$  Lyrae consist of two series, one with a bright field and dark wires, and the other with a dark field and bright wires; those of 61 Cygni were made with the dark wires only. Before the beginning of these measurements an arrangement was attached to the tube of the micrometer for adjusting the stellar focus, so that this adjustment could be deliberately and carefully made, and during the work care was taken that this adjustment should be always exact. In the case of our 26 inch refractor the tube of the micrometer is shifted one-thirtieth of an inch during the year, it being pushed in during the cold weather and drawn out in summer. It has been assumed in all our previous reductions that the coefficient of temperature for the screw of our micrometer is zero. To test this assumption, which would have an important influence on the reductions, at the same time that the observations for parallax were going on a series of measurements was made on a pair of stars in the cluster of  $h$  Persei. These measurements were not reduced until the observations for parallax were finished, and on account of the previous result of a zero coefficient for temperature, the measurements were not so numerous as they might otherwise have been; still they show a

decided correction for change of temperature.

The observations for parallax were arranged so that the micrometer and the wires were reversed on each night, and at the end of an observation the wires were restored to their first position. Each of these observations being reduced to a chosen epoch by correcting for proper motion, precession, nutation and aberration, gives an equation of condition of the form

$$x + by + cz + du + n = 0,$$

where  $x$  is the correction to the assumed difference of declination,  $y$  the correction to the assumed value of the annual proper motion,  $z$  the difference of the constants of aberration for the two stars,  $u$  the difference of the parallaxes of the stars, and  $n$  is the residual between the observed difference of declination and an assumed value. The assignment of weights to such equations is a delicate matter, and whenever possible I have preferred to give the weight unity to each of them, notwithstanding the fact that the observations were made under very different conditions of the images. The solution of each set of equations by the method of least squares gives the following values of the parallax:

*$\alpha$  Lyræ.*

Dark wires,  $\pi = 0''.1556 \pm 0''.00764$ .

Bright "  $\pi = 0.2080 \pm 0.00827$ .

Taking the mean of these results by weight, we have the final result:

$$\pi = 0''.1797 \pm 0''.00561.$$

Time of light = 18.11 Julian years.

*61 Cygni.*

Dark wires,  $\pi = 0''.4783 \pm 0''.01381$ .

Time of light = 6.803 Julian years.

The substitution of these values in the equations of condition shows that the greatest residual in the case of  $\alpha$  Lyræ is for the dark wires  $0''.120$ ; and for the bright wires it is  $0''.160$  there being 69 equations in the first case and 59 in the second. The observations of 61 Cygni are not quite so

good, the greatest residual here among the 63 equations being  $0^{\circ}.209$ .

The star 61 Cygni has one of the largest and best determined parallaxes that we know of, since a recent investigation by Dr. ELKIN has reduced that of  $\alpha$  Centauri to nearly the same value. This quantity being half a second of arc, it can be seen easily with a good telescope. But a tenth of a second is so small that one can hardly distinguish it even with the highest magnifying powers, except with remarkably steady and distinct images. To determine a parallax of this amount we must trust therefore that in a large series of measurements the influence of the parallax will be shown in the mean result. And my own experience has convinced me that such will be the case, if the observer be careful, and has practiced so long that his habit of observing will not change during the period of observation.

#### A LARGE METEOR.

DANIEL KIRKWOOD.

A very brilliant meteor passed over central Indiana on the evening of January 3, 1883. It was seen by many persons in Bloomington, but unfortunately I was not of the number. The following description was furnished at my request by F. E. HUNTER, Esq., who saw the meteor from its first appearance until it passed behind a building at an altitude of  $15^{\circ}$  or  $20^{\circ}$ :

"At about 6:57 p. m., January 3d, 1883, I saw a meteor in the northern sky which was unusually brilliant. It started about  $20^{\circ}$  east of the *North* star and at nearly the same altitude, going in a westerly direction. It was at first about the second magnitude but increased to the first magnitude before going  $10^{\circ}$ . It passed about  $6^{\circ}$  or  $8^{\circ}$  below the *Pole* star, being at that point about one-third the size of the full moon. The sky was misty, with light clouds, but the light was almost that of half-moon, being of a light green hue. At a point under the *Pole* star it seemed to be retarded in its progress for an instant, and to lose some of its brilliancy, but I attribute it to a cloud which obstructed

the view, although it looked like mist. Just below and little to the west of the *Pole* star a beautiful train about  $15^{\circ}$  long was visible for half a minute after the meteor disappeared. Only a short train was visible behind it as it moved, the head of the meteor seeming to leave a blaze *immediately* behind it, with a train probably  $6^{\circ}$  or  $8^{\circ}$  long. The head together with the tail was cone-shaped. I saw it traverse probably  $60^{\circ}$  or  $70^{\circ}$  when it went behind a building. Judging from the duration of the light I suppose it exhausted itself about  $10^{\circ}$  from the horizon, keeping in the direction in which it was moving while I could see it. It was visible about fifteen or twenty seconds, moving apparently very slowly. The *North* star could be seen plainly but the sky was not clear. I heard no sound and as I did not see it disappear I do not know whether it exploded or not. It was light enough to recognize a person across the street."

A special telegram from Bloomington, Illinois, to the Indianapolis Journal says:

"A very brilliant and beautiful meteor was observed about 6:30 this evening. It was first seen near *Jupiter*, and flashed in a south-easterly direction across the sky, lighting up everything for several seconds with the greatest brilliancy. The head of the meteor had the brilliant, clear appearance of an electric light, followed by a train of brilliant blood red, much like the trail of a rocket."

The following notices are taken from the Cincinnati Gazette:

KOKOMO, Ind., January 3.—A magnificent heavenly display was witnessed in this section at 7 o'clock to-night, in the shape of a meteor, or flying star. It was first seen at a point directly overhead, and passed in a westerly direction and disappeared, leaving a train of light behind which could be seen for several minutes. It resembled a large ball of fire and was perfectly blue. It created considerable of a stir on the streets.

CHICAGO, January 3.—At a quarter before 7 this evening, a large meteor, resembling the electric light in brilliancy

and color, lighting the entire heavens, shot from a point  $45^{\circ}$  above the horizon in the south-east, downward and westward, being extinguished suddenly in the south west, about  $10^{\circ}$  above the horizon. A dark, red light remained in the sky at the point of starting several minutes afterward. This light at first elongated in the direction taken by the meteor, gradually fading to an irregular rounded form.

Special to the Commercial Gazette:

INDIANAPOLIS, January 3.—A brilliant meteor passed over this city this evening about 7 o'clock. Proceeding from the constellation of the Harp, it pursued a south-westerly direction, moving with great velocity, and leaving a line of light in its wake sufficiently bright to illuminate the sky like the moon when crescent.

Mr. H. B. VORIS, who observed the phenomenon at Waveland in the southern part of Montgomery county, Ind., says the meteor "was due west when it disappeared." Professor J. L. CAMPBELL, of Crawfordsville, who was in Indianapolis at the time, saw the disappearance a little *north* of west, while Mrs. CAMPBELL, who saw it at Crawfordsville, says it was a few degrees *south* of west. Rev. W. P. McNARY pointed out to the writer the position at disappearance as seen from Bloomington, Ind., which was about N.  $55^{\circ}$  W., at an altitude of nearly  $20^{\circ}$ . These observations agree in fixing the termination of the meteor's visible path over the southern part of Champaign county, Illinois. The estimated angles of elevation are rather widely discordant. I find, however, that the meteor became visible over Grant county, Ind., at a height of about eighty-five miles; that it passed very nearly over Kokomo and Lafayette, its height at the latter place being fifty-three miles; that its explosion or disappearance occurred without any sound at an elevation of thirty-three miles; that its course was S.  $78^{\circ}$  W; and that the length of its visible track was about 140 miles. Observers differ so widely in their estimates of the time of flight that the nature of the meteor's orbit cannot be determined with any certainty.

BLOOMINGTON, Ind., January 1883.

ON THE PRESENT STATE OF THE THEORIES OF  
THE CELESTIAL MOTIONS.

SIMON NEWCOMB.

[In the introduction to the first volume of the Astronomical papers of the American Ephemeris, Professor NEWCOMB has given a masterly sketch of the present state of our knowledge as to the celestial motions, and of the directions in which research should be prosecuted. Such *resumes* are of the highest value, and we print this in order to make it reach the widest circle of readers. EDITOR.]

It is well known to all astronomers who have given attention to the subject that meridian observations of the moon and planets are not completely represented by any of the existing tables, and that the deviation of prediction from observation is constantly increasing. It is true, that so far as the current requirements of astronomy are concerned, the state of the case may be considered as not unsatisfactory. Not only may the planets be found and eclipses predicted for many years to come by the present tables, but, with the exception of the moon, there is every reason to suppose that the tabular positions will serve the purposes for which they are immediately required in navigation and practical astronomy. But when we take a wider view and consider the general wants of science both now and in the future, we find that in the increasing discordance between theory and observation there is a field which greatly needs to be investigated.

If mutual gravitation according to the law of NEWTON is the only cause which changes the motions of the planets, then it is mathematically possible to construct tables which shall represent observations with the last degree of precision and through any period of time. It is quite possible that the discordances alluded to proceed solely from the imperfections in the mathematical theory, and do not indicate any unknown cause affecting the celestial motions. But when we investigate more closely, and seek to ascertain the cause of such discordances, we find a state of things which renders it impossible to draw any definite conclusion re-

specting the ultimate possibility of representing observations by existing physical and mathematical theories. This state of things has its origin in the comparative brevity of the period during which accurate observations have been made, and in the difficulty of conducting, on a systematical plan, mathematical investigations having in view the perfection of astronomy.

One point in which the requirements of astronomy differ from those of physics is that the element of time enters into the former much more than into the latter. The experimental investigation of forces which act on the surface of the earth requires only the time necessary to make and perfect the experiments. There is no one research of which we can say that it will necessarily require a definite number of years or centuries for its completion. But since astronomical generalizations rest, not upon experiments, but upon observations, it is always necessary to wait for the recurrence of the phenomena on which the conclusions are to depend. The main object of investigation being the forces which change the motions of the planets we must observe these motions during a sufficient period to make evident the action of the forces. The longer the time which elapses the more material we have for reaching conclusive results. It is generally considered that accurate observations commenced with BRADLEY in the middle of the last century. The period during which they have continued is therefore about a century and a third. But there are many exceptions in the case of different classes of observations. The places of the moon have been traced backward with a nearly modern precision through the century preceding BRADLEY'S observations, while the observations of the Babylonians and the Arabs are still of the greatest value in the lunar theory. On the other hand none of BRADLEY'S instruments fulfill the requirements of the present time, and his observations were in many cases extremely defective as compared with our own. If, therefore, we attempt to learn what conclusions can be reached in the present state of astronomy we must consider each object of observation-



separately with reference to its general place in a comprehensive scheme.

But time is not the only element which comes in. If we are to determine what unknown causes affect the motions of the planets the first step is to prove that there is really a discordance between the results of observations and the results of the theory of gravitation. The first step towards establishing such a discordance is the construction of tables and formulæ of which we can say that they are beyond reasonable doubt the results and the only results of the gravitation of the known bodies of the solar system. The necessary conditions which such tables and formulæ must satisfy are that they shall be founded upon uniform elements and data, and that the results of employing the adopted elements shall be carried out with all necessary precision. Now, not only has this requirement never been fulfilled, but the effect of recent advances in exact astronomy has rather been to carry us away from its fulfillment.

It is scarcely possible for a year to pass without some new investigation, or series of observations which shall materially add to the precision with which we can determine some astronomical constant. Each astronomer who finds material to be used in this way, is naturally desirous of utilizing it to its fullest extent, and is therefore under a temptation to introduce each new improvement into his investigations without respect to their consistency with the investigations of others which have been made with the older data. Sometimes, too, the object of constructing an astronomical formula is to correct it from time to time, and the very object of the constructor may tend to destroy its consistency. A brief glance at some features of the existing planetary tables will illustrate the point in question.

LAPLACE, in the third volume of his *Mecanique Celeste*, constructed by the most rigorous and complete methods then known to science, a complete theory of the planetary perturbations, founded on elements and masses which are quoted in Chapter VI of his work. From his results tables were constructed by LINDENAU and BOUVARD during the early years of the present century.

In order to give the tables the required precision it was necessary to correct the elements by a comparison with observation. Thus, the new tables no longer correspond to the original formulæ of LAPLACE. Moreover, the theory was in many respects so imperfect that no certain conclusion could be drawn from a comparison with observation. This was notably the case with the perturbations of the second order. It was therefore necessary to make a complete reconstruction of the theory. Nevertheless, such was the labor and difficulty of constructing new tables that those of LINDENAU and BOUVARD remained the standards for use in the preparation of ephemerides during nearly half a century.

The next complete reconstruction of the theories and tables of the planetary motions was that of LEVERRIER. His work on this subject forms the most important part of the fourteen volumes which he published under the title *Annales de l'Observatoire de Paris*. The first of these volumes appeared in 1855, the last in 1877.

Some considerations of the circumstances under which this great work was carried out, and of the objects at which it aimed may not be out of place as showing how it happens that more remains to be done in the same direction. When LEVERRIER commenced his work, the most striking feature which presented itself was the imperfections of the tables of LINDENAU and BOUVARD. The formulæ on which they were constructed, though fully up to the science of the time in which they were formed, was far behind modern requirements in generality and rigor. Better tables and formulæ constituted one of the most pressing wants of exact astronomy. Both his position and his previous works marked LEVERRIER as the one to undertake the work of constructing such tables and formulæ. Naturally desirous of beginning to reap the results of his labor as soon as possible, he investigated the elements of the planets and published the corresponding tables one or two at a time. This course did not detract from his main object, that of constructing improved planetary tables. But there was another object, the desirableness of which was not immediately

felt, but which must be more and more felt in the not distant future, namely, the attainment of uniformity in adopted astronomical data. So far was LEVERRIER from aiming at this object, in its entirety, that his tables do not, in all cases, embody his final results. The consequence is, that notwithstanding that his work makes a greater epoch in astronomy than any of his immediate successors can hope to make, it does not wholly supply the wants of science in the immediate future. In many of his tables large and increasing deviations from observation already exhibit themselves. This is most notably the case with the planet *Saturn*, the theory of which he did not succeed in bringing to a satisfactory conclusion. The geocentric places of *Mars* and *Venus* are also largely in error at the time of the nearest approach to the earth. The earlier tables those of the *Sun* and *Mercury*, are the only ones which can be regarded as entirely satisfactory in their agreement with observations, with the possible exception of *Uranus* and *Neptune*.

What has been said of LEVERRIER'S tables applies with greater force to the tables of *Uranus* and *Neptune* by the present writer. Their main object was to supply an immediate astronomical want. The data on which they were found could not be regarded in any respect as definitive, nor were the adopted masses absolutely uniform. The formulæ of perturbations on which they depend are also such that we cannot say with certainty whether the deviations from observations which they exhibit arise from any other cause than the imperfections of the theories on which they are founded.

Now, the material available for the accurate determinations of the fundamental elements of astronomy has increased many fold since the conclusion of LEVERRIER'S work on the four inner planets. The recurrence of transits of *Venus* and *Mercury*, the perfection of astronomical instruments, the employment of improved places of the fixed stars, the introduction of more systematic methods of research, and the reinvestigation of older observations have all combined to bring precise astronomy to a higher plane than it ever before occupied. Supposing that their mutual

gravitation is really the only cause which disturbs the elliptic motion of the planets around the *Sun*, it is now theoretically possible to construct tables of all the large planets, except *Neptune*, from exact data, which shall represent observations within their probable errors until the middle of the next century. The desirableness of having such tables founded on one consistent and fully elaborated theory, hardly needs to be insisted on. Only in this way can it be decided whether deviations from theory arise from its imperfections, or from the action of unknown and, perhaps, unsuspected causes.

A more detailed survey of the field will bring to light other reasons for placing the results of past observations and researches in such a form that they may be utilized in the future.

We first remark that the existing data in the form of observations lie in great part unused, and are in danger of never being used, unless discussed and condensed in such a way as to render them manageable. Long series of observations made during the present century by eminent astronomers, and with the best appliances, lie idle in the volumes which embody them, never having appeared in any of the existing tables. In order to be utilized to the best extent they need to be rediscussed by modern methods and with modern places of the fixed stars. The labor of doing this is such that we only find it performed in sporadic cases by individual astronomers. One of two courses must now be adopted. We must either suffer this great mass of material collected in many cases by the life labors of eminent observers, and published at great expense, to go to utter waste, or we must speedily put it in a shape to be utilized for present and future purposes. It is true that if nothing were to be added to the mass we might safely leave it in confidence that future astronomers would give it more attention than we have. But so rapidly does it increase that it is even now entirely beyond the power of individual management, and the longer it is left the less hope there is that it ever will be managed. The required work must be that of an organization rather than that of an individual.

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DETROIT, MICH.

*PRESENT THEORIES OF THE CELESTIAL MOTIONS. 17*

All that the head of an organization can do is to plan the work, investigate the formulae and data by which it is to be done, devise the checks which are to guard against error, discuss the results, arrange them for the press, and see that every operation is conducted on correct principles and by the best methods.

Not only should the work be founded on all the observations which it is practicable to employ as its basis, but a necessary feature is a utilization, so far as possible, of all discussions by other astronomers. Although the work may become less individual in character, it has greater claims to consideration on the score of embodying the labors of the leading astronomers of the time.

On assuming the superintendency of the American Ephemerides in 1877, the writer determined to employ the resources at his disposal to carry out, or at least to enter upon, a long cherished plan of executing the work in question. No published announcement of his programme was, however, made, owing to the ease of making such a programme alongside the difficulty of executing it. There are, however, two reasons for no longer maintaining this reserve. One is that although what has been done is only a commencement, the prospects of being able to carry it through are fairly good. Both Congress and the Navy Department have supplied all the assistance which has been asked for, and a force of from eight to twelve computers, some of the highest order of mathematical ability, has been actually employed during the past year, and may, if necessary, be increased in the future. Another and more cogent reason for announcing the programme is that much duplication of work may thus be avoided. Astronomers in other parts of the world are from time to time undertaking investigations already in hand and sometimes announce their intention in private correspondence where nothing has appeared in print.

*(To be Continued.)*

The new comet discovered June 23, at the Puebla observatory, Mexico; was seen by English observers January 27.

It was then faint, about 4' in diameter with but little central condensation.

### 61 CYGNI.

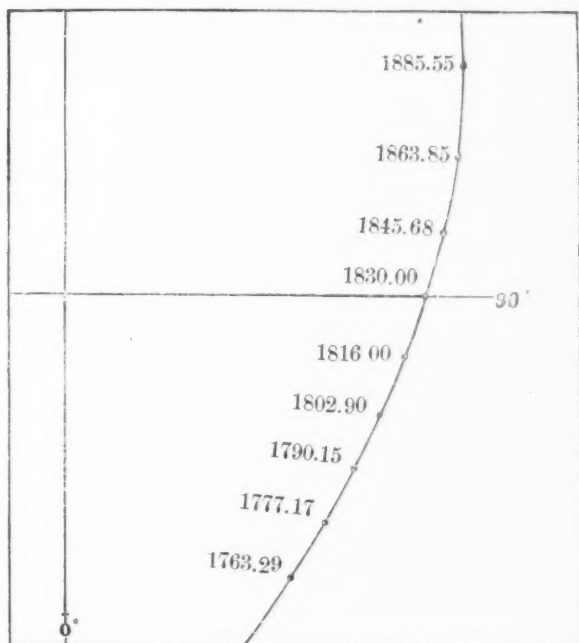
N. M. MANN, ROCHESTER, N. Y.

Great interest attaches to this star because of all the doubles thus far observed with the exception of *a Centauri* it has the most sensible parallax. This interest is increased by the doubt that has lately been thrown upon its being a binary. SECCHI, in the article translated for Johnson's *Cyclopædia* (1878), gives it up, referring to recent investigations, presumably those of WILSON, to which NEWCOMB also attaches much weight in his *Astronomy*. Thinking that a fresh study of this object with the aid of the latest observations might yield more satisfactory results, I have devoted to it such patient and careful labor as my leisure for the last few months would allow. The observation of which I have been able to avail myself are 182 in number, beginning with BRADLEY's in 1753. As the results of numerous approximations and comparisons, the following positions were finally reached (angles reduced to equinox of 1880):

| ANGLES. | DISTANCE. | EPOCHS. |
|---------|-----------|---------|
| 40°     | 15."73    | 1763.29 |
| 50°     | 15."02    | 1777.17 |
| 60°     | 14."69    | 1790.15 |
| 70°     | 14."75    | 1802.90 |
| 80°     | 15."08    | 1816.00 |
| 90°     | 15."78    | 1830.00 |
| 100°    | 16."86    | 1845.68 |
| 110°    | 18."28    | 1863.85 |
| 120°    | 20."12    | 1885.55 |

These positions charted,  $3\frac{1}{2}$  millimeters to the second, give the points as represented below. The curve, especially in the latter part, which also is the part best covered by

observations, is unmistakable. The ellipse is easily completed.



## 61 CYGNI.

Scale  $3\frac{1}{2}$  millimeters to the second.

Three observations only have been rejected: LALANDE's, in 1793 (which compared with HERSCHEL's in 1781 and PIAZZI's in 1800 is more than  $10^\circ$  astray); LINDENAU's in 1813, and STRUVE's in 1814, which are mutually contradictory and consist only of  $\Delta$ . R. A. Careful comparison of the calculated with the observed angles in the remaining 179 cases gives very satisfactory results. These comprise all that are given in the "Hand-book of Double-Stars" and in the last report of the Naval observatory, together with

my own observations made during the progress of this work. The following table shows in compact form the result of this comparison:

TABLE OF DIFFERENCES ( $\mu_o - \mu_c$ ) SHOWING RESULTS BY PERIODS.

| $\Delta$             | +3.3 | +2.1 | +1.2 | +1.5 | +1.4 | +1.3 | +1.2 | +1.1 | +1.0 | +0.9 | +0.8 | +0.7 | +0.6 | +0.5 | +0.4 | +0.3 | +0.2 | +0.1 |
|----------------------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|
| No. of observations. |      |      |      |      |      |      |      |      |      |      |      |      |      |      |      |      |      |      |
| 1753-1816            |      | 1    |      |      | 1    |      |      | 1    |      |      |      |      |      |      |      |      |      |      |
| 1816-1830            |      |      |      |      |      |      |      |      |      |      |      |      |      | 1    |      |      | 1    | 2    |
| 1830-1845            |      |      |      |      |      |      |      |      |      | 1    |      |      | 1    | 1    | 1    | 1    | 1    | 3    |
| 1845-1864            | 1    |      |      |      | 1    |      |      |      |      | 1    |      | 2    |      | 2    | 2    | 3    | 4    | 8    |
| 1864-1883            |      | 1    |      |      |      | 3    | 1    |      | 1    |      |      |      |      | 1    | 6    | 4    | 6    | 8    |

| $\Delta$             | 0.0 | -0.1 | -0.2 | -0.3 | -0.4 | -0.5 | -0.6 | -0.7 | -0.8 | -0.9 | -1.0 | -1.1 | -1.2 | -1.3 | -1.4 | -1.5 | -1.6 |
|----------------------|-----|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|
| No. of observations. |     |      |      |      |      |      |      |      |      |      |      |      |      |      |      |      |      |
| 1753-1816            |     |      |      | 2    |      | 1    |      |      |      |      |      |      |      |      |      |      |      |
| 1816-1830            |     | 1    |      |      | 1    |      |      |      |      |      |      |      |      |      |      |      |      |
| 1830-1845            | 5   | 3    | 7    | 3    | 1    |      | 1    | 1    |      |      |      |      |      |      |      |      |      |
| 1845-1864            | 3   | 10   | 2    | 4    | 3    |      |      | 1    |      | 1    | 2    |      |      | 1    | 1    |      | 1    |
| 1864-1883            | 8   | 7    | 3    | 3    | 7    | 3    | 2    | 2    | 1    | 1    |      | 1    | 2    | 1    |      |      |      |

The few observations earlier than 1820 are so discordant in measurements of distance that they must mostly be set aside as of little value. Comparison of my calculations with observations since 1820 gives results which are condensed as follows:

TABLE OF DIFFERENCES ( $P_o - P_c$ ) SHOWING RESULTS BY PERIODS.

| $\Delta$             | +0.9 | +0.8 | +0.7 | +0.6 | +0.5 | +0.4 | +0.3 | +0.2 | +0.1 | 0.0 | -0.1 | -0.2 | -0.3 | -0.4 | -0.5 | -0.6 | -0.7 | -0.8 | -0.9 |
|----------------------|------|------|------|------|------|------|------|------|------|-----|------|------|------|------|------|------|------|------|------|
| No. of observations. |      |      |      |      |      |      |      |      |      |     |      |      |      |      |      |      |      |      |      |
| 1820-1830            |      |      |      |      |      |      |      |      |      |     | 1    | 1    | 1    | 2    | 2    |      |      |      |      |
| 1830-1845            |      |      |      |      |      | 1    | 1    | 1    | 3    | 5   | 5    | 5    | 4    | 5    | 2    |      |      |      |      |
| 1845-1864            |      |      |      |      |      | 3    | 8    | 7    | 8    | 4   | 6    | 4    | 4    | 3    | 1    |      |      |      |      |
| 1864-1883            |      | 1    | 2    | 2    | 1    | 8    | 16   | 11   | 6    | 6   | 2    |      | 1    | 1    |      |      |      |      |      |

The calculations of distance are from a fixed point; but as these stars are not very unequal, the principal star must itself describe a considerable ellipse, and therefore the observed distance will necessarily vary from the calculated until this lesser ellipse is also determined. Possibly some



law of this kind may be seen in the way the line of mean differences slopes through the table. When the comparison is toward either apsis we should expect the observed to exceed the calculated distance, since the principal star is also then toward the opposite apsis of *its* ellipse. This is just what we find in this case. For the last twenty years, as in the early observations, the observed distance is in excess.

Since writing the above my eye has fallen on Mr. JAMES M. WILSON'S reference to this star in "A Hand-book of Double-Stars" before referred to. The date of the article is 1876. He says:

"A little consideration will show that this orbit cannot be elliptical. Taking the early observations into account the companion has described about  $80^\circ$ , and yet has scarcely deviated from a rectilinear path. It is almost certain that the relative path is of a hyperbolic nature."

He then gives a figure, drawn to a scale of seconds, representing approximately the course of the *comes*. It is almost a straight line, and certainly could not be brought into an ellipse with any reference to the central star. It seems to me that Mr. WILSON has given too much weight to measurements of distance. His angles and epochs alone would give a different result. As far as they go they call for a greater curve than I have given, as will be seen by the following comparison. The first column of angles is WILSON'S, the second is mine:

|      |                     |                     |
|------|---------------------|---------------------|
| 1820 | 82. <sup>o</sup> 7  | 83. <sup>o</sup> 0  |
| 1825 | 86. <sup>o</sup> 4  | 86. <sup>o</sup> 6  |
| 1830 | 89. <sup>o</sup> 9  | 90. <sup>o</sup> 0  |
| 1835 | 93. <sup>o</sup> 3  | 93. <sup>o</sup> 8  |
| 1840 | 96. <sup>o</sup> 5  | 96. <sup>o</sup> 9  |
| 1845 | 99. <sup>o</sup> 6  | 99. <sup>o</sup> 9  |
| 1850 | 102. <sup>o</sup> 6 | 102. <sup>o</sup> 8 |
| 1855 | 105. <sup>o</sup> 5 | 105. <sup>o</sup> 5 |
| 1860 | 108. <sup>o</sup> 2 | 108. <sup>o</sup> 2 |
| 1865 | 110. <sup>o</sup> 8 | 110. <sup>o</sup> 8 |
| 1870 | 113. <sup>o</sup> 3 | 113. <sup>o</sup> 2 |
| 1875 | 115. <sup>o</sup> 7 | 115. <sup>o</sup> 5 |

The difference is slight, only  $0.^{\circ}5$  in 55 years, but it is on the wrong side for his theory.

Following are the elements which I have reached by THIELE's graphical process:

|               |   |   |   |   |   |                 |
|---------------|---|---|---|---|---|-----------------|
| $T$           | - | - | - | - | - | 1899.13         |
| Period        | - | - | - | - | - | 1159 years.     |
| $\Omega$      | - | - | - | - | - | $328.^{\circ}$  |
| $\pi$         | - | - | - | - | - | $125.^{\circ}4$ |
| $\gamma$      | - | - | - | - | - | $51.^{\circ}15$ |
| $\lambda$     | - | - | - | - | - | $56.^{\circ}43$ |
| $\varepsilon$ | - | - | - | - | - | 0.2484          |
| $a$           | - | - | - | - | - | $31''.59$       |

According to this, taking the latest measurements of parallax as given by PROCTOR,  $0''.55$ , the combined mass of the two stars is only one-seventh of our sun; which is an interesting fact as contrasted with the enormous mass of *Sirius* and (by inference) of the first magnitude stars which show no sensible parallax.

ROCHESTER, N. Y., Jan. 1883.

#### DOUBLE STARS.

1882.

| Star      | Epoch,<br>mean | Hour Angle<br>h m | Position,<br>O | Dist.,<br>" | No of Obs | Power | Rem'ks          |
|-----------|----------------|-------------------|----------------|-------------|-----------|-------|-----------------|
| Struve    | 1321 1882.312  | +3 0              | 60 20          | 19.40       | 2 489     |       |                 |
| Struve    | 1263 1882.309  | +2 2              | 19 50          | 40.30       | 2 "       |       |                 |
| (A and B) |                |                   |                |             |           |       |                 |
| Struve    | 1001 1882.290  | +4 45             | 65 22          | 8.53        | 1 "       |       | Bad Definition. |
| Struve    | 1009 1882.292  | +5 27             | 152 51         | 2.70        | 2 "       |       |                 |
| Struve    | 1187 1882.280  | +1 36             | 50 30          | 1.88        | 2 "       |       |                 |
| Struve    | 389 1882.313   | +4 27             | 64 34          | 2.38        | 2 "       |       |                 |
| Struve    | 343 1882.213   | +5 56             | 326 2          | 25.90       | 1 "       |       |                 |
| Struve    | 1428 1882.200  | -2 6              | 87 50          | 2.93        | 2 "       |       | Good seeing     |
| Struve    | 1588 1882.200  | -3 14             | 55 30          | 14.25       | 3 "       |       |                 |
| Struve    | 1771 1882.200  | -4 26             | 78 30          | 1.60        | 2 "       |       |                 |
| Struve    | 1602 1882.191  | -3 1              | 178 26         | 14.20       | 1 "       |       |                 |
| O. Struve | 299 1882.312   | -1 42             | 25 50          | 3.09        | 1 "       |       |                 |
| Struve    | 1334 1882.334  | +2 21             | 238 40         | 2.56        | 3 "       |       |                 |
| Struve    | 1553 1882.340  | +1 2              | 169 15         | 5.52        | 3 "       |       |                 |
| O. Struve | 199 1882.189   | -1 32             | 116 58         | 4.70        | 2 "       |       |                 |
| O. Struve | 235 1882.189   | -2 37             | 68 0           | 1.40        | 2 "       |       |                 |
| O. Struve | 233 1882.181   | -2 41             | 337 12         | 4.20        | 1 "       |       |                 |

|                     |      |          |          |        |         |     |  |
|---------------------|------|----------|----------|--------|---------|-----|--|
| Struve              | 1543 | 1882.301 | +1 33    | 5 23   | 5.29 1  | " " | { this dist. is ev-<br>id'ly too small |
| (A and B)           |      |          |          |        |         |     |  |
| Struve              | 1516 | 1882.265 | +1 25    | 93 28  | 10.48 3 | " " |  |
| Ursa Majoris        |      |          |          |        |         |     |  |
| O. Struve           | 196  | 1882.274 | +2 44    | 35 90  | 9.60 2  | " " |  |
| Struve              | 1306 | 1882.310 | +2 5 243 | 10     | 2.38 3  | " " |  |
| Struve              | 1934 | 1882.430 | -0 39    | 32 30  | 5.88 1  | " " | Poor Definition                        |
| Struve              | 2199 | 1882.389 | -3 32    | 100 47 | 1.52 2  | " " |  |
| Struve              | 2218 | 1882.430 | -1 35    | 348 50 | 1.98 2  | " " |  |
| Struve              | 2032 | 1882.432 | -0 40    | 203 14 | 3.42 2  | " " |  |
| Struve              | 1984 | 1882.405 | -6 10    | 276 24 | 6.32 2  | " " |  |
| Struve              | 2034 | 1882.427 | -6 16    | 115 13 | 1.37 2  | " " |  |
| Struve              | 1280 | 1882.323 | +0 8     | 41 40  | 5.30 2  | " " |  |
| O. Struve           | 312  | 1882.392 | -5 50    | 141 30 | 4.77 2  | " " |  |
| (B and C)           |      |          |          |        |         |     |  |
| Struve              | 2278 | 1882.408 | -7 10    | 146 8  | 5.97 1  | " " | Seeing very bad                        |
| (A & B) 39 Draconis |      |          |          |        |         |     |  |
| Struve              | 2323 | 1882.408 | -5 0     | 2 20   | 3.73 2  | " " |  |
| Struve              | 2271 | 1882.410 | -4 40    | 263 40 | 2.36 1  | " " |  |
| O. Struve           | 296  | 1882.410 | -3 2     | 312 28 | 1.47 2  | " " |  |
| $\mu$ Draconis      |      |          |          |        |         |     |  |
| Struve              | 2130 | 1882.412 | -5 20    | 164 40 | 2.81 3  | " " |  |
| 44 Bootis           |      |          |          |        |         |     |  |
| Struve              | 1909 | 1882.411 | -4 10    | 241 15 | 4.49 1  | " " | images unsteady                        |
| Struve              | 2192 | 1882.420 | -3 57    | 69 40  | 10.38 3 | " " | "Good observat'n                       |
| Struve              | 2054 | 1882.373 | -4 20    | 359 45 | 1.16 2  | " " |  |

37 Total

These observations were made with the 8.25 inch Equatorial. All the observations for position have been corrected for *Zero*, by allowing the star measured to traverse the position web.

F. E. SEAGRAVE.

Not long ago Mr. C. W. TALLMAN, of Batavia, N. Y. purchased of ALVAN CLARK & SONS, a portable telescope having an object-glass of five inches aperture with magnifying powers ranging from 25 to 500. The instrument cost \$400. The mounting was done by Mr. TALLMAN, himself, and it is said to be novel in design and convenient in manipulation.

Professor J. M. DEGARMO of DeGarmo Institute, Rhinebeck, N. Y., by kindness of friends becomes the possessor of a Fitz equatorial refractor of six inches aperture. It is used for instruction and some physical observation.

## EDITORIAL NOTES.

The total eclipse of the *Sun*, May 6, 1883, is a topic of special interest at the present time, because it will offer another favorable opportunity for observation of the corona and chromosphere of the *Sun*. The eclipses of 1878 and 1881 suggested new and important questions relating to the solar atmospheres that astronomers and physicists are eager to answer. The coming eclipse is peculiarly favorable, on account of the duration of the total phase. The average time of totality of eclipses of the *Sun* is usually about two minutes; the May eclipse will continue for six minutes.

Unfortunately the narrow strip of territory from which alone this eclipse is visible traverses the South Pacific Ocean nearly from side to side, just falling short of the solid ground for an astronomer to stand on in South America or in Australia. With great ingenuity, also, the track of the eclipse just misses nearly all of the small Pacific islands, including, in fact, only two small coral reefs named Flint and Caroline Islands, respectively. To the latter of these (which must not be confounded with the Caroline Islands, which are further west), an expedition is shortly to be sent, some account of which may be interesting.

The expedition is sent by the United States Government, by the co-operation of the National Academy of Sciences, of the Naval Observatory, and of the Coast Survey. The party expects to sail from New York March 1, from Panama March 9, and from Callao (Peru) March 22. A Government vessel (probably the *Iroquois*) will convey the astronomers direct from Callao to Caroline Island (which is in 10 degrees south latitude and 150 degrees west longitude). They should arrive about April 20 or 25, and the time until May 6 will be spent in preparations.

The island is a mere reef, or collection of small islands, which contained in 1874 only some thirty native inhabitants, and one stray Englishman. So far as is known it has not been visited since 1874.

When the eclipse is over the party will return home via Honolulu and San Francisco, after a voyage of some 12,000 miles by sea and a land journey of 3,000 miles. The party consists of Prof. HOLDEN, director of the Washburn Observatory at Madison, Wis., who intends to examine the vicinity of the *Sun* for the detection of a planet nearer the *Sun* than *Mercury*; of Prof. HASTINGS, of the Johns Hopkins University, who will make spectroscopic observations of the solar corona; of Mr. ROCKWELL, of Tarrytown, N. Y., who will observe the contacts and make eye observations; of Prof. C. S. PIERCE, of the Coast Survey, who will also make spectroscopic observations; of Lieut. BROWN, of the Navy, and of two photographers who will be sent by the Royal Society of London, to photograph the corona and its spectrum.

The party is well equipped with instruments in every way, and from the skill of its various members and from the unusual opportunities which they have, we have every reason to look for important results. The first news from this party will probably come from San Francisco about the first of next June. Late advices say that Professor E. S. HOLDEN will have charge of the Caroline island expedition having been appointed chief astronomer of it.

Professor E. C. PICKERING has recently issued the accompanying circular of instructions, for observers of variable stars, acting in co-operation with the Harvard College Observatory. Carleton College Observatory is to participate in this work, and will gladly supply pamphlet copies of the *Plan for Securing Observations of the Variable Stars* to any observers desiring the same. The MESSENGER heartily commends this excellent plan, and calls special attention to the following instructions:

A considerable number of observers have signified their intention of joining in the work, proposed in the pamphlet entitled "A plan for Securing Observations of the Variable Stars." Since several of these observers are as yet without the requisite experience, some precautions are necessary to ensure such uniformity of method in the observations as will enable them readily to be reduced and compared together. The following recommendations, partly repeated from the pamphlet above named, are intended to assist in effecting this purpose. Other recommendations will be sent from time to time to the observers taking part in the work, if it seems desirable to do so.

It has not seemed best, however, to urge that all the observations should be made and recorded in exactly the same way, and to furnish printed forms for the records. The difference of instruments and of the circumstances under which the observations are to be made would probably make it difficult to secure precise uniformity. But there are some conditions which can be everywhere fulfilled, and the neglect of which will seriously impair the value of the work.

The time of each observation (hour and minute) should be recorded, and the date and place should be entered on each separate sheet of paper used in the records, as well as the signature of the observer.

For the completeness of an observation, it is necessary that the star observed should be compared both with a brighter and with a fainter star. This may be done by either of the methods described in the "Plan for Securing Observations of the Variable Stars;" that is, either by estimating the difference in brightness between the star observed and each of the others separately, or by estimating its brightness upon a scale of 10, in which the number 0 represents the light of the brighter, and 10 that of the fainter comparison star. The method adopted should be stated, as the abbreviated form of the record may otherwise produce confusion. The estimates themselves may be made with photometric apparatus if the observer prefers; but he will then do well to add estimates made without the photometer, since very little time will be required to do so, as the star is already under observation. It may happen that the photometric observation will differ to some extent from the estimate.

When the stars observed are readily visible without a telescope, it is best to dispense with the instrument. Similarly, a large telescope should not be used when a small one is sufficient. Neglect of this

principle may seriously impair the accuracy of the observations.

Great care must be taken to identify with certainty the stars which are observed. It is obvious that an observer who has failed in doing so will practically lose all the time he afterwards employs in making estimates of relative brightness. Stars visible to the naked eye may be identified from any good atlas. The *Atlas Cœlestis* of Heis is especially to be recommended. American observers can obtain it through any importing bookseller at a cost of about nine dollars. The *Durchmusterung* (by ARSELANDER) is the chief means we have for the identification of telescopic stars in the northern hemisphere. Observers unable to obtain it may often find it in a public library. With sufficient pains on the part of the observer, a telescopic star may be correctly identified by its place with regard to some star visible to the naked eye.

The comparison-stars used must be clearly described by their positions with reference to well known stars, or to the star with which they are compared, either in words or by a drawing showing the positions of the stars in the region, unless they have been identified from a catalogue, in which case the numbers of the catalogue suffice. In the record of the observations the comparison-stars may be conveniently designated by letters of the alphabet. In any set of comparison-stars, to be used with a given star requiring observations, the same letter should always be assigned to each on the different occasions when the region is examined.

The star requiring observation should also be described by its position with regard to the neighboring stars, so that it may be known that the proper star has been observed.

It will not be necessary to repeat this work of identification on every occasion. When it has once been accomplished, and the region is known, a reference to the previous identification is sufficient in subsequent observations.

In any case, if a star is found equally bright with that to be observed, the statement of the equality should be made, and furnishes one complete observation. But several independent observations on each date are highly desirable when they can be furnished.

If any one of the stars to be observed becomes too faint to be followed with the means at the disposal of the observer, notice of the fact should be sent at once to the Harvard College Observatory, in order that observations may be attempted with larger instruments.

In order to obtain satisfactory results the observers are requested to pay close attention to the methods of observation described on pp. 10-13 of the "Plan for Securing Observations of the Variable Stars," and to the form of record recommended on pp. 13, 14. The description of the stars employed in each region will probably require a separate sheet of paper. On the sheets containing the record of the comparisons, a blank line should be left at the end of each series, and the

full designation of the next variable star examined should have a line to itself at the beginning of the comparisons relating to it. In these comparisons themselves it may be designated simply by the letter *V*.

Observers are also requested to make reports of their progress every month, so that additional instructions may be promptly sent if they are required. Even if no observations have been made since the last report, the statement of the fact will be of use in showing the actual state of the undertaking at given times.

EPHEMERIS OF THE GREAT COMET, *b*, 1882.

(Communicated by Vice Admiral Rowan, Supt. U. S. Naval Obs'y.)

Computed from elements (Nature 688) and reduced to the mean Equinox 1883.0 Greenwich mean noon.

|      |      | R. A. |    |      | Decl. |    |    | Log. <i>r</i> . | Log. $\Delta$ |
|------|------|-------|----|------|-------|----|----|-----------------|---------------|
|      |      | h     | m  | s    | °     | '  | "  |                 |               |
| 1883 |      |       |    |      |       |    |    |                 |               |
| Feb. | 10.0 | 6     | 0  | 37.8 | —19   | 41 | 17 | 0.48137         | 0.38891       |
|      | 14.0 | 5     | 57 | 40.4 | 18    | 40 | 13 | 0.48909         | 0.40520       |
|      | 18.0 | 5     | 55 | 19.7 | 17    | 41 | 17 | 0.49669         | 0.42132       |
|      | 22.0 | 5     | 53 | 32.7 | 16    | 44 | 35 | 0.50413         | 0.43723       |
|      | 26.0 | 5     | 52 | 14.7 | 15    | 50 | 14 | 0.51133         | 0.45282       |
| Mar. | 2.0  | 5     | 51 | 24.4 | 14    | 58 | 16 | 0.51841         | 0.46817       |
|      | 6.0  | 5     | 50 | 58.7 | 14    | 8  | 43 | 0.52532         | 0.48322       |
|      | 10.0 | 5     | 50 | 54.8 | 13    | 21 | 37 | 0.53200         | 0.49790       |
|      | 14.0 | 5     | 51 | 12.3 | 12    | 37 | 0  | 0.53861         | 0.51231       |
|      | 18.0 | 5     | 51 | 47.9 | 11    | 54 | 52 | 0.54508         | 0.52635       |
|      | 22.0 | 5     | 52 | 39.5 | 11    | 15 | 10 | 0.55135         | 0.53995       |
|      | 26.0 | 5     | 53 | 46.1 | 10    | 37 | 56 | 0.55751         | 0.55316       |
|      | 30.0 | 5     | 55 | 6.1  | 10    | 3  | 6  | 0.56354         | 0.56594       |
|      |      |       |    |      |       |    |    |                 |               |
| Apr. | 3.0  | 5     | 56 | 38.1 | 9     | 30 | 34 | 0.56944         | 0.57828       |
|      | 7.0  | 5     | 58 | 20.9 | 9     | 0  | 19 | 0.57520         | 0.59015       |
|      | 11.0 | 6     | 0  | 13.9 | —8    | 32 | 21 | 0.58090         | 0.60158       |

WASHINGTON, Feb. 10, 1883.

E. FRISBY,  
Prof. Math., U. S. N.

NOTE.—In the published elements  $\varphi$  should be  $89^\circ 13' 42''.70$ , instead of  $89^\circ 7' 42''.70$ .

TRANSIT OF VENUS.

(Extract from a Letter of Rev. F. Perry, S. J.)

Our first month at Nos Vey, (Madagascar), was all sunshine, but a fortnight previous to the transit this was succeeded by a cloudy and rainy season, brought by the north winds. Fortunately the sky cleared during the nights of December 3 and 4, and so we obtained excellent star observations for getting accurate local time. The night of Dec. 5 and the morning of Dec. 6 were again cloudy, but the wind changed

shortly from north to south, and this brought a strong wind that cleared the heavens, and gave us a splendid opportunity for observing the transit. Capt. ALDRICH, R. N., observed with the stonyhurst, 4 in., JONES' equatorial, and the Rev. W. SIDGREAVES and myself used the 6 inch instruments provided by the Government Transit-of-Venus-Committee. The instruments were all protected as well as possible from the south gale by reed erections south of the huts, and by canvas. The internal contact was well observed at all three instruments, and the principal phase shows an extreme range of about five seconds of time. The atmosphere of *Venus* was well seen by all observers, and the internal geometrical contact of the atmosphere of the planet with the *Sun's* limb was timed by all. A shower of sand, due to the strong south wind, was a little in the way, but we could altogether have scarcely been more fortunate. Our longitude depends on an excellent run, with 9 chronometers, made by Capt. ALDRICH, R. N., from the Cape of Nos Vey and back, calling at Durban each way. The time at the Cape was telegraphed by Dr. GILL to Simon's Bay, and he has also sent us time signals at Durban, where Mr. PERR, on our outward trip, and Mr. NELSON, F. R. A. S., on our return, have been of great assistance. Mr. NELSON has just accepted the post of Director of the new Government Observatory at Natal, and is already in possession of a good 8-inch equatorial, by GRUBB. I am leaving our sidereal clock, DENT, 1915, for this observatory. Our visit to Madagascar occurred at a rather inconvenient time, as the natives had murdered some white men shortly before our arrival, and, suspecting that we had come to punish their treachery, were less friendly than they might otherwise have been. The only village at Nos Vey was nearly deserted almost as soon as we settled there, and we found it somewhat difficult to obtain provisions. Fear being their ruling passion, we were left unmolested, under the protection of H. M. S. *FAWN*, but acts of outrage on unoffending Europeans in our neighborhood occurred only a day or two before our departure.

The evening of Dec. 6 was fine, and we obtained good star observations on that and the following nights. In the day time we were rapidly packing our instruments, and when the last star had been observed at 10 p. m., on Dec. 8, we at once dismounted our altazimuter and sidereal clock, and were ready to sail at 6 a. m. on December 9.

P. S. I say above that our longitude depends on a chronometer run, but I have neglected no means of strengthening the result by other observations. Lunar altitudes were taken with the altazimuter, both by the Rev. W. SIDGREAVES and myself, and I obtained a goodly harvest of occultations during November, having observed 17 on one night alone. Dr. GILL, Director of the Capetown Observatory, Colonel TUPMAN, in New Zealand, and others, proposed to take part in these observations, and there is little doubt that the results will be very sat-



isfactory. The determination of the latitude was, of course, a matter of very little difficulty, considering the excellent altazimuter at our disposal.

Some time ago attention was called to the observations and drawings of the great comet of 1882, made by Mr. J. R. HOOPER, of Baltimore. It was not convenient, at that time, to prepare the cuts showing the peculiar changes in the terminus of the tail, to which he gave careful attention, so they are presented now (though late), as an important feature of the observational record. We select the following from full notes and drawings kindly furnished us:

1882. Oct. 2, 4:30 A. M. Find the dark streak less noticeable nearer the nucleus, but extended out further towards the tail. A second dark streak towards end of tail and next the faint or N. edge. Terminus of tail still well defined, and this has been very much marked throughout. The nucleus seems to subtend a less angle, with axis of tail, say  $7^\circ$  or  $8^\circ$ , and also shows a separation two-thirds towards the tail.

Oct. 5, 5:15 A. M.—The upper dark streak has disappeared; end of tail seems to be broader and has a bifurcation. Nucleus more elongated, shows a bright central condensed point, which has a dark space of separation between it and portion towards tail. Dark space following nucleus much less apparent.



Oct. 7, 4:45 A. M.—Find the bright central point divided cleanly into two parts. This was also clearly and independently seen by Dr. CHAS S. HASTINGS; 4.1-in. glass power 71. The bifurcation changed in form. Diagram made.

Oct. 7, 5 A. M.

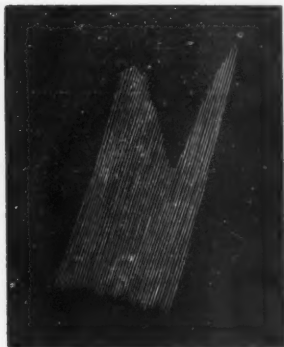
Oct. 10, 5 A. M.

Oct. 10, 5 A. M.—The dark rift not seen this morning. Bifurcation

further changed as per diagram herewith. Nucleus continuous only faintly condensed near center, and fades more gradually into the coma.

Oct. 15, 4:35 A. M.—Only condensed point in nucleus very near head-end of comet. Bifurcation very clearly defined in finder; envelopes not seen.

Oct. 16, 4:45 A. M.—Decided lengthening of lower limb of bifurcated tail.



Oct. 16, 4:45 A. M.



Oct. 25, 4:30 A. M.

Oct. 25, 4:30 A. M.—Nucleus fades into coma. The lower limbs of tail is broader and longer. The tail seems to be narrower and not so much curved.

Oct. 30.—Moonlight and cloudiness prevented definition of any detail. These observations are condensed from my book, are made with 5-in. CLARK equatorial powers, 45 to 107, and with 4.1-in. eq., made by Prof. C. S. HASTINGS, power 70. The 4.1-in. glass has the finest definition of any I have ever seen. While observing on the morning of Oct. 7, the air was so still that we tried the glass on *Sirius* and could see its companion. The position of *comes* has been described by more than one person, not previously aware of it, during last winter. I mention this to show we made no mistake.

STANDARD TIME.—The Secretary of State has recently addressed a circular to our representatives abroad, asking them to refer to the different governments the question whether it is deemed desirable to hold an international conference for the purpose of establishing a common meridian to which longitudes shall be referred. This circular letter is written in accordance with a resolution adopted by Congress at its last session, authorizing the President to call a convention of representatives from the several nations for this purpose. Answers of acceptance, it is announced, have been received from Mexico and

Venezuela, while Belgium is reported to be unfavorable to the project. Switzerland and Hawaii favor the plan. In France the matter was referred to the Academy of Sciences, and was by the Academy submitted to a commission composed from the sections of Astronomy and Geography.

W. U.

CHRONOMETRIC LONGITUDES.—The *Comptes Rendus* for Jan. 8, 1883, contains an interesting note by M. DE MAGNAC, upon the accuracy of longitudes determined by chronometers. A comparison is made with the values determined in 1871-1873 of the longitudes of Bahia, Montevideo and Rio-de-Janeiro, with the telegraphic values more recently obtained by officers of the U. S. Navy. The differences are as follows:

Chronometric — Telegraphic.

|                 |      |
|-----------------|------|
| Bahia,          | -1.3 |
| "               | +1.0 |
| Montevideo,     | -0.5 |
| Rio-de-Janeiro, | -1.1 |

This surprising accuracy, for expeditions of over forty days, is due to the method adopted—that of M. VILLARCEAN, in which the rates observed on the land before departure, and after the return are made the basis of a calculation giving the rate from day to day as a function of the time and temperature.

W. U.

Feb. 2, a message was received from Harvard College Observatory announcing the discovery of a minor planet, by J. PALISA, director of the Naval Observatory at Pola, Austria. Its position, Feb. 1, 1883, Greenwich mean time, was:

R. A.  $-10^h\ 5^m\ 10^s$ .

Decl.  $=+9^\circ\ 49'\ 45''$

Its daily motion in R. A. is  $-44''$ ; in Decl.  $7'$  north, 12 magnitude.

Professor LEWIS SWIFT, of the Warner Observatory, announced the discovery of a bright comet Feb. 23, three degrees north of the star *Beta Pagani*. Its tail is thirty minutes long, and the motion of the comet is slowly eastward. At the same time, though bearing a later date, a telegram was received from Harvard College Observatory announcing the same discovery.

R. A.  $= 22^h\ 50^m$ .

Dec.  $= +23^\circ$  Motion eastward.

#### JUPITER—BLACK TRANSIT OF SATELLITE III.

I observed on the night of February 12, at  $9^h\ 40^m$  *Jupiter's* third satellite transiting the planet as a small very black spot—smaller than

its shadow generally is. Its visible path lay about half way between the south pole and the southern edge of the heavy band just south of the equator. The satellite was watched until midway across, when observation of its progress was discontinued. At this time, IV was at superior conjunction close above the north pole of the planet. The planet was not looked at again until 12<sup>h</sup> 30<sup>m</sup>, when III had left the disk some distance, and appeared about as bright as II and I—certainly as bright as the disk on which it had appeared as a black spot. I witnessed black transits of this moon on two former occasions, viz: August 2d, 1879, at about 15<sup>h</sup>, and December 30, 1880, at about 8<sup>h</sup>.

E. E. BARNARD.

NASHVILLE, Tenn., Feb. 15, 1883.

Professor T. H. SAFFORD, Director of the Williams College Observatory recently reports that "At Williams we are now giving the calculation of orbits as a regular exercise to bachelors of arts, who continue astronomy the year after their graduation, and I think before many years it will become a senior elective. I am quite convinced that the way to reform the mathematical instruction of this country is to make astronomy more prominent in our higher elective courses, rather than to let our young men lose themselves altogether in the maze of quaternions and the modern higher algebra."

Just so, especially when young men are so anxious to get at something more practical and less theoretical. At Carleton College one senior in the course of arts has already computed a parabolic orbit for the great comet of 1882, and he is now trying better observations for elliptical elements.

A late issue of *Knowledge* says "Professor YOUNG expresses his belief that even were a comet, whose mass equaled the earth's, to run straight into the *Sun*, the effect would only be to add to the *Sun's* store of heat, not to increase the emission of heat. Mr. M. WILLIAMS expressed the same opinion in the *Gentleman's Magazine*. It is probably correct. The heat generated by the downfall of such a comet on the *Sun's* surface would be employed in producing changes of physical condition, and given out afterward as the matter so changed resumed its original state."

Late reports from all the Government parties for transit-of-*Venus* observations, indicate successful work. The weather was most unfavorable at Washington, yet 50 good photographs were taken there.

Mr. R. B. GANS, optician at Columbus, Mo., is a maker of equatorial telescopes. He has shown us drawings of his work, with descriptions of the same, that indicate excellence.

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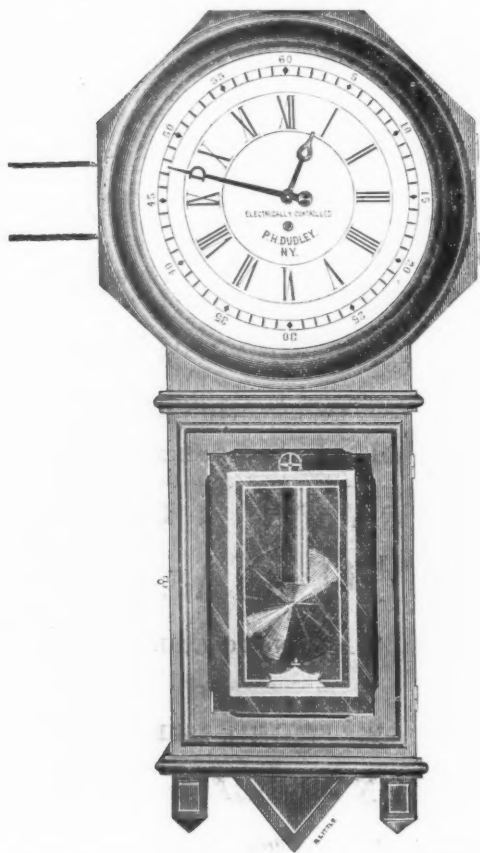
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